first part consists of 200 pages, comprising the families Ranunculaceæ to Aquifoliaceæ, but, unfortunately, we have to wait for the concluding part of the work for the appearance of the introduction and key to the families. Without these the "Flora" loses some of its value and much of its interest, and it is to be hoped that the publication of the succeeding parts will take place as rapidly as may be possible.

The plan followed in the "Flora" is that adopted by Prain in his "Bengal Plants," and is a plan admirably suited for a local flora where the easy identification of the plant is the object in view. Descriptions of species are therefore omitted, and the whole flora is in the form of key. A description of the natural family is succeeded by a key to its genera. Each genus is concisely described, and a key to its species follows, and then under each species there is no further descriptive matter, but only geographical and economic information and vernacular names. In those genera represented by only a single species, a short description is given. The keys are well drawn up, and a good test of their efficacy is to be seen in the genus Impatiens with its seventy species, which are all clearly differentiated. It should be mentioned that Mr. Gamble was assisted by Mr. S. T. Dunn in the preparation of about the first 132 pages of this part.

The Theory of Abstract Ethics. By T. Whittaker. Pp. viii + 126. (Cambridge: At the University Press, 1916.) Price 4*s*. 6*d*. net.

THIS book is the result of stimulus applied, as the author informs us, by Prof. Juvalta's "Old and New Problem of Morality." Though awakened from dogmatic slumber by Renouvier, Mr. Whittaker had continued, in accordance with English tradition, to try to derive the ethical law of justice from "ends" or "goods." But the *a priori* cannot be avoided; and if a metaphysical doctrine emerges that is more in harmony with the moral aspirations of mankind, we must not refuse to consider it out of a forced austerity.

The fundamentals of every moral system are liberty and justice; and abstract ethics, as distinguished from the art of life in general, is a kind of impersonal science of the conditions under which all the types are bound to live in common. In the present state of affairs, however, the author naturally expatiates into concrete ethics and politics, giving a useful summary of Kant's view. The moral law recognised within states should be extended to their mutual relations, with the aim of eternal peace, which will be possible when we have progressed to a permanently superior political society. But he did not postulate a world-state so much as a family of states each respecting each other's individuality. Finally, on the last page, the author permits himself a legitimate speculation, perhaps too friendly, in the direction of reincarnation, which is certainly one feasible way of resolving many moral problems.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Method of Curves.

THE expression of the results of observations and experiments by curves became common during the first half of the nineteenth century. One of the first instances was given by Perkins (*Phil. Trans.*, 1826) in a paper on the compressibility of water.

Six years later Sir John Herschel (Trans. Ast. Soc., v., 1) gave an account of the method of graphical construction on squared paper as applicable to astronomical computations and physico-mathematical inquiries.

"The dates in years and decimals are measured as abscissæ, and the angles in degrees and decimals as ordinates. The next step is to draw by the mere judgment of the eye, and with a free but careful hand, not through but among the points, a curve presenting as few and slight departures from them as possible, consistently with the character of large and graceful sinuosity, which must be maintained at all hazards.

"But since an equal trustworthiness can probably not be placed on all the observations, we must take care to distinguish those points which correspond to observations entitled to the greatest confidence, such as those which appear to have been made under peculiarly favourable circumstances, or which rest upon the average of a very great number of individual measurements. These should be marked on the chart in some special manner not liable to be mistaken, and when we draw the curve we must take care to make it pass either through or very near all those points which are thus distinguished; or at least to deviate from them with much more reluctance than from such as have no claim to our peculiar attention.

"By substituting the curve for the points we have made a nearer approach to nature, and in a great measure eliminated errors of observation."

A few years later Regnault (Mem. Acad. Sci., 1847, xxi., p. 316) reduced the method to a fine art. To represent the expansion of mercury he used four copper sheets, 80 cm. square, each divided into 10,000 squares. Within these squares values were marked by a special dividing engine, one bevelled edge of the heavy base of which was graduated into 8 mm. divisions and tenths. A carriage running on a half-millimetre screw, the large head of which was divided into 50, so that 0·01 mm. could be accurately measured, carried the burin. Experimental values were marked by the intersections of lines drawn by the burin. A free curve was drawn by Regnault, which was completed and engraved by an artist. Even with these precautions a constant error was detected in the last plate.

The introduction of the copper plate and dividing engine seems to conduce to the accuracy and permanence of the record.

The method has been rendered more easy of application and possibly more accurate by the introduction of mechanically ruled paper, a good sample of which of French manufacture consists of sheets a metre square, ruled into millimetre squares, each edge of which is divided into 0·2 mm. by dots. Free hand-curves have also been more or less replaced by mechanically cut curves and flexible laths.

Notwithstanding the very general use of the method and many theoretical accounts of it (Whewell, "Nov. Org. Ren.," 1858, p. 204; Stanley Jevons, "Principles of Science," 1877, p. 492), culminating in the admirable reports of Prof. Hele-Shaw (B.A., 188–92) "On Graphic Methods in Mechanical Science," there still seem to be many doubtful points in the theory and practice of the process; much valuable information has never been published, and is confined to individual workers, while the few attempts which have been made to estimate the accuracy attainable have given widely different results.

The following questions seem to present themselves among others for consideration.

What is the best material for a diagram sheet?

Mechanically ruled paper is by far the most generally used, but it is not very permanent and is apt to be injured by the points of measuring instruments. Possibly the best material would be ordinary, white, or blue glass, which alters very little with time, has a low coefficient of linear expansion, $<0{\cdot}000\,009$, and is not easily scratched. The requisite lines could be marked by a diamond, carborundum wheel, or special ink; or the whole plate might be varnished, the lines then drawn on the varnish and etched in.

Does the colour of the sheet or ink make any difference in the accuracy or ease of the work?

Babbage found that black on green conduces to ease and accuracy in the use of tables. Chocolate on white is said to be more legible than black on white.

Is it more advantageous to work with lines as fine as consistent with visibility, or always to the same edge of thicker and more visible lines?

Is there a limit of size, say, about a square metre, beyond which increase in size does not conduce to accuracy?

What is the best method of measuring lengths on diagrams? What is the effect of time and damp on paper sheets and of change of temperature on metallic ones?

A difference of 10° C. in the temperature of the room would alter the length of a copper sheet by 0·00017, but this is corrected by using the sheet as the measuring instrument.

What is the best form of lath? Wood, steel, or steel backed by lead? How should the lath be held or pinned?

In what cases are other forms of ruling, such as semi-logarithmic, logarithmic, triangular, or circular, advantageous?

By general consent the curve selected should show as few changes of curvature as possible consistently with passing through or near the great majority of the experimental points and lying fairly among them. Suppose one or more points lie at a considerable distance from the curve—is this due to experimental error and to be therefore neglected?—to a rapid but continuous change in the condition of the substance under examination, to be represented by a change of curvature, or to a change in the nature of the substance to be represented by a break and a new curve?

The answer to these questions depends upon the estimate which the experimenter forms of the "error" of his experiments. One may consider his error as large, and prefer a simple curve which does not represent his results very exactly; another may deem his error less, and prefer a more complicated curve passing more nearly among the experimental points; a third may consider that his error is very small, and that his results are best expressed by two or more simple curves, and hence assume a very fundamental change in the nature of the substance.

In very accurate work, then, the experimenter is more or less obliged to estimate or determine the error of his observations, and much has been written on methods for the purpose. Most experimenters seem not to repeat their experiments several times under as nearly as possible the same conditions, without which no determination of the error is possible, but trust to subsequent correction by the curve. The "probable error" is generally the most convenient; it may be obtained from a considerable number (n) of observations upon a single quantity by finding the residuals (v), that is, the excess or defect of each observation from the arithmetical mean, adding the squares of the residuals together, dividing the sum of the squares by $n\ (n-1)$, and multiplying the square root by 0·67449, or $p.e. = 0{\cdot}67449\sqrt{\Sigma v^2/n(n-1)}$.

The estimates of the accuracy attainable are, as might be expected, very various. It is stated (J.S.C.I., xxii., 1227) that a density determination, such as that of dilute nitric acid, can be carried to 1 part in 75,000, and this claim is moderate.

On the other hand, it is curious to find (Clarke's Tables, 298) that the results for the density of chloroform found by a great recent experimenter at two different temperatures each differ by about 1 part in 2500.

It is perhaps not so generally recognised that the graphical method itself introduces a fresh series of errors which may be quite comparable in magnitude with, or even greater than, those incidental to careful experiments.

Every graphical reduction comprises five operations, each liable to error—measurement of the abscissæ, measurement of the ordinates, drawing the curve, measurement of the abscissa, and of the ordinate of the new value required. Hele-Shaw remarks that the results given by the use of graphical methods cannot be regarded as very accurate, and quotes Poncelet and Culmann:—"The constructing engineer will give preference to geometrical solutions whenever an accuracy of results up to three decimals (one-thousandth), which can be perfectly well obtained, is sufficient." By mechanical engineers about 1/2000 seems to be considered the limit of accuracy. To take the simple case of ordinary rectangular co-ordinates, the draftsman depends upon the accuracy of the machine ruling. Suppose an ordinate is 1′ out of the perpendicular, the measured abscissa is too long or too short by 1/3400 of the length of the ordinate.

It is extremely difficult to make a valid estimate of the error introduced by a graphical reduction, depending as it does upon individual eyesight and hands. Good eyes can distinguish a tenth of a millimetre between two points, but age, accompanied as it too often is by astigmatism, may much impair this estimate.

Stanley Jevons attempted to find the value of π by the careful use of compasses; he did not come nearer than 1/540. He does not mention which of the numerous approximate constructions he used.

To obtain the probable error of the experiments and reduction, the square root of the sum of the squares of the separate sets of residuals must be taken.

The adequate estimation of the errors, both of the results and the reduction, becomes of still greater importance when it is attempted to establish breaks in the curve and discontinuity in the results by obtaining differential coefficients from the equation to the curve, by plotting differences, or by mechanical means (Proc. R.S.E., May, 1904). It must also be remembered that each of these processes introduces a new series of errors of its own, and may apparently increase the original errors, which are more or less removed by the first curve.

Each experimental result is represented by a point, and however much the scale of the diagram is enlarged these points remain points, and may give a false appearance of accuracy. In very accurate work would it not be worth while to extend Herschel's suggestion and determine the probable error of each experimental result? Each result could then be expressed by a circle the radius of which is equal to half the probable error, and which would increase with the size of the diagram. If another experiment be made under similar conditions it is about an equal chance that it falls within or without the circle, which therefore affords a measure of the precision of the observations. Since there is little evidence against any curve which cuts the circle, the variations in size might profoundly modify the opinion of the draftsman as to the direction of his curve.

Sydney Lupton.

Ground Rainbows.

I have seen with pleasure Mr. Heath's clear and instructive letter and diagrams on this subject in Nature of March 2. Some fourteen years ago I calculated the altitudes of the sun required to produce the elliptic and other arcs, and obtained results in agreement with Mr. Heath's, except that I took 41° instead of 42° for the semi-angle of the cone.

For Petersfield, at 11 a.m. on October 14, 1915, the sun's altitude, 23°, appears to be somewhat underestimated, and I make it just above 30°, but this, of course, leaves the bow still hyperbolic.

I was led to consideration of the curves for the ground rainbow when seeking for a reason why the sky rainbow is seen always circular, though, when the sun is not on the horizon, the bow might perhaps have been expected to appear elliptical, the circle being projected into an ellipse on a plane perpendicular to a sight-line, assumed horizontal.

I came to the conclusion that, there being no definite plane of reference in the sky, and the rays being parallel, there is, as it were, no element of definite distance involved, so that the sky bow always appears circular. But for the ground bow we have a definite horizontal plane of reference, so that this bow becomes a conic section, varying with the sun's altitude.

I had some interesting correspondence at the time with the late Sir G. G. Stokes, and I may perhaps quote from one of his letters, dated August 22, 1902, only six months before his death. Replying to my question as to whether a dew bow is seen as a circle or an ellipse, he wrote:—

"It is a question of the combination of sensation and expectation. In a dew bow we are impressed with the idea that the luminosity we see is spread over a horizontal plane; and we tacitly ask ourselves the question: What must be the actual form of the locus of the drops on the grass in order that the luminosity may appear as it does? The answer, of course, is, an ellipse, or it might be an hyperbola. If the question be: As what do we see the bow? the answer depends on a combination of sensation with interpretation of sensation. If we merely saw the luminosity, and knew absolutely nothing about its history, we should never think of anything but circularity about it."

I have often looked for a ground bow, but have never been fortunate enough to see one.

Observing a fine lunar rainbow on January 21, I found the light to be polarised in planes passing through the point looked at and the radius at the point, just as is the case with the solar rainbow. I hope that Mr. Heath will test the next ground bow with a Nicol prism.

C. T. Whitmell.

Invermay, Hyde Park, Leeds, March 3.

In the Proceedings of the Royal Society of Edinburgh, vol. vii. (1869–70) Clerk Maxwell has a short note on a bow seen on the surface of ice. This was observed on January 26, 1870, on the frozen surface of the ditch which surrounds St. John's College, Cambridge. Maxwell remarks, "How a drop of water can lie upon ice without wetting it and losing its shape altogether I cannot profess to explain." In 1898, in vol. xxii. of the same Proceedings (1898) there is a note on dew bows by Dr. R. A. Lundie and myself. These were produced at night on the ground, the source of light being the gas lamp or electric light of the street. A short account will be found in Nature of January 12, 1899 (vol. lix., p. 263).

C. G. Knott.

Royal Society, Edinburgh, March 4.

Science and the State.

Referring to Prof. Cohen's letter in Nature of March 2, it may not be untimely to cite another paragraph written in 1831 *re* neglect of science in this country. Sir David Brewster, in his "Life of Newton," published in that year, says:—

"But what avails the enthusiasm and efforts of individual minds in the intellectual rivalry of nations? When the proud science of England pines in obscurity, blighted by the absence of the royal favour, and of the nation's sympathy—when its chivalry fall unwept and unhonoured—how can it sustain the conflict against the honoured and marshalled genius of foreign lands?"

The position to-day is fortunately not quite so bad as here indicated by Brewster, but is it not still the case that, in the words of Sir Archibald Geikie, science rests under an incubus of apathy and indifference? Expansion of science and national evolution are two matters that in the opinion of the writer are intimately bound up one with the other. Neglect of the former really means inhibition of political progress.

David Balsillie.

Greyfriars Garden, St. Andrews, March 4.

THE NATIONAL IMPORTANCE OF THE DYE INDUSTRY.

At the annual meeting of the Bradford Dyers' Association held on February 28 the chairman of the directors, Mr. Milton S. Sharp, made a highly interesting statement on the national position with regard to the supply of dyes. He described with great force and clearness the close connection between the manufacture of dyes and high explosives, and pointed out how Germany by reason of her huge, highly organised, and ably administered colour works, producing all the raw materials for the making of high explosives, was able immediately to divert much of their plant to war purposes. He paid a high tribute to Lord Moulton and the High Explosives Department for their services, the value of which, he said, the country will probably never know, in improvising the manufacture of high explosives. He urged that whatever it involves, we must establish the aniline dye industry in this country, so that in case of war we may have the ability to produce quickly any amount of high explosives the Army or Navy may need. The extensions of plant that have been made for the temporary purpose of manufacturing high explosives will, he says, make a long and essential step towards the colour industry, and to break them up after the war would be little short of criminal folly. Mr. Sharp quoted some effective examples of German activity in relation to the chemical service of the war. He alluded to one